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Interactive comment on "Reconstruction of super-resolution fields of ocean $p\mathrm{CO}_2$ and air–sea fluxes of CO_2 from satellite imagery in the Southeastern Atlantic" *by* I. Hernández-Carrasco et al.

Anonymous Referee #2

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Hernandez-Carrasco et al present an interesting new approach to map the partial pressure of CO2 in the surface ocean and the resulting air-sea gas flux, using satellite data. The authors convincingly show that their new high resolution approach obtains better results than a low resolution product (CARBONTRACKER) in the Benguela system when being compared to in-situ observations. The manuscript offers a method to the reader that on the one hand can be used to monitor the carbon cycle in the important EBUS regions but further has the potential to be applied globally.

I do believe the manuscript offers (a) a novel approach, (b) is clearly written – particu-

larly the method section is easy to follow for the reader – and (c) describes an approach with potential for many future applications, hence I do recommend the manuscript for publication in BG. My specific comments below are intended to further improve the manuscript:

specific comments:

General: I only have one overarching point of criticism and this is the choice of data. While the authors do a great job testing several satellite chlorophyll-a and sea surface temperature products, the more fundamental question is why temperature and chlorophyll alone? E.G. it becomes very clear when looking at figure 11 (see longitudes 12.5 to 13.5 differences >20 μatm) that there is a stronger in-situ to product disagreement close to shore. Is this not a sign that near the coast the available data streams are possibly not enough to capture all the variability, whereas the more open ocean areas are better represented? At least some discussion would be useful.

Abstract lines 1-4: circular sentence – remove or revise

Introduction:

General: In the introduction there is a use of GHG's and CO2. The manuscript itself has its focus on CO2. Is the intention to motivate the reader that this approach can be used for all GHG's (then please state so explicitely)? Otherwise for clarity the use of GHG may be replaced by CO2

page 1407 line 6: "resolve" not "solve"

page 1407 line 8: "prevent us"

page 1407 lines 19-20: Your products big advantage is its high resolution. It seems unfair in the introduction to present the 4x5 degree monthly climatology from Takahashi et al as the most "advanced" pCO2 based product in this respect. There are high(er) temporal resolution products (Rödenbeck et al 2014 – 4x5 degree daily) and spatial resolution products (Nakaoka et al 2013 – 0.5x0.5 degree monthly; Landschützer et al

2014 - 1x1 degree monthly), which I think fir better in this discussion. This however does not change the message as the product presented in this study is still of higher resolution.

Page 1408 lines 10-11: I am not convinced that this statement is true for the ocean (at least not as much as it is for the land)

Data:

page 1410 line 23: “ENVISAT” - throughout the manuscript, some abbreviations are explained (e.g. SCIAMACHY), whereas others (like e.g. ENVISAT) are not

page 1411 lines 21-24: Globalview reports xCO2 in the atmosphere, whereas you report oceanic pCO2. Please clarify how you have dealt with this difference (unlike the fCO2 to pCO2 correction, the xCO2 to pCO2 correction is not minor, hence it is not necessary neglectable when you compute air-sea fluxes, i.e. it has to be explicitly shown)

Method:

page 1418 lines 13-17: Please consider splitting this sentence in two to make it easier to read.

Results:

Although the merged products provide more coverage, the missing data from cloud coverage provide a major limitation to the product especially when air-sea fluxes of CO2 and their variability are investigated. This is a problem on the local, as well as on the global scale. In view of the future applications the authors mention, how do you plan to deal with this issue?

Figures:

I was a bit puzzled looking at figure 1: Both products illustrate a strong carbon uptake along the coast (purple color) whereas I would have expected the opposite.

Figure 6d: Is this the average flux density (averaged by latitude)? I think the integrated flux (in GtC/s or TgC/yr, etc.) is a better visualization than the flux density and it additionally makes it easier to put the importance of the sink into a bigger (regional/global) perspective.

Figures 7 and 8: Why is there a difference between the estimated area here and in figure 1?

Interactive comment on Biogeosciences Discuss., 12, 1405, 2015.